The Institute of Physics

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The Bulletin is published by The Institute of Physics for circulation to its members.

Notices to Members

Annual Dinner

The second Annual Dinner of the Institute will be held at the Park Lane Hotel on Tuesday, May 5, 1959. Further details will be announced later. Members are asked to note the date ; there is no restriction on the number of guests that may be brought.

Electronics Group Autumn Meeting

The 1958 Autumn meeting of the Electronics Group will be held in University College, London, Gower Street, W.C.1. The subject of the meeting is " Solid state memory and switching devices " and will cover ferrite, magnetic thin films, superconductive, and ferro electric devices, together with considerations of design requirements in such devices. Sessions have been arranged for 3.00 to 6.30 p.m. Friday, September 26, and 10.00 a.m. to 12.30 p.m. Saturday, September 27. Further details will be available shortly from the Institute.

Second Report on the Availability of Places for Physics Students

Reprints of the report published in the June *Bulletin* are available and may be obtained by members free of charge. Application should be made to the Deputy Secretary.

The names of several colleges recognized for Ordinary National Certificates in Applied Physics and from which information had been obtained were unfortunately omitted from the list given in Appendix II of the report. The correction has been made in the reprint.

The Cybernetic Approach to Mentality and Society*

By W. GREY WALTER, M.A., Sc.D.

(*Burden Neurological Institute*)

THE meaning attached to the word Cybernetics by Wiener is somewhat different from that proposed by Ampère, who defined it rather more literally in relation to the mechanism of Government. The sub-title to Wiener's epoch making book is "The Science of Control and Communication in the Animal and Machine." In the series of conferences organized by the Josiah Macy Junior Foundation, the general title "Cybernetics" was elaborated in the sub-title as "Circular, Causal and Feedback Mechanisms in Biological and Social Systems." Ashby in his recent work *An Introduction to Cybernetics* has described his impression of the subject as "the study of systems that are open to energy but closed to information and control."

There is no reason, however, why these usages should be separated. In so far as the behaviour of an individual machine or animal depends upon communication and inter-action between its various components or organs, the individual may legitimately be considered as a compact society in which the conditions for stability and survival of the whole may be analysed in the same terms as those relating to social systems comprising a number of complex individuals.

The proposition that Society may be considered as if it were a complex machine is not in itself novel—in fact this assumption is implicit in all systematic approaches to economic problems—the novelty is rather in the greater confidence with which machines can be defined and analysed since the introduction of modern electronic devices, whether for the performance of set tasks or the demonstration of theoretical notions. As Ashby emphasizes, the theoretical importance and practical value of Cybernetics do not depend merely on the transposition of principles from one science to another, though this is often the most striking novelty. The significance of the cybernetic ways of thinking is rather that they encourage, even enforce, the study of *behaviour*. That is, a cybernetician is disciplined to plan attacks on problems which are invulnerable to simple experimental weapons, problems in which the active elements cannot be controlled or eliminated.

The elementary principles of cybernetic analysis have been familiar to physiologists since the reflex activity of the nervous system and the homeostatic regulation of the internal environment were first studied, several generations ago, notably by Claude Bernard and Walter Cannon. The enlargement of the field of view attributable to cybernetics is due mainly to the acceptance of the need to study several systems simultaneously, and to work out methods for recording and correlating functions of several variables. Classical physiology attempted to follow the traditions of the physical sciences in limiting experiments to the study of a single variable by ruthless dissection and isolation of single organs. It was not realized, for example, that two systems, each inherently stabilized by reflexive "negative feedback" circuits may become unstable when coupled together, and the principles of communication between such systems, though implicit in many physiological axioms, rarely received explicit consideration. Nor was it realized how complex could be the behaviour of a system with only a few elements, provided that the elements were free to communicate with one another, even on a binary or "all-or-none" channel.

Another difficulty familiar to biologists, but until recently only rarely appreciated as a factor in the study of Society, is the presence in most complex systems of powerful *non-linear* operators. Such effects are usually referred to in physiological contexts as thresholds. This notion is so commonplace in everyday life that it easily goes unnoticed, but whether noticed or not, a threshold element enormously complicates the behaviour of any system in which it is found. This particular difficulty is considered by Ashby in both his published works, but is often passed over by those who conider Cybernetics merely as a respectable rendezvous for otherwise illicit intercourse between the Sciences. The obvious way in which the presence of a non-linear element can render a

* The substance of a lecture to the South-Wales Branch of the Institute given in Cardiff on March 14, 1958

system intractable to classical analysis is that the ordinary forms of differential calculus are thereby rendered impotent. For example, the differential equations set up by Keynes to describe the relation between investment, interest rate and the circulation rate of money assume that the rates of change linking these various parameters have a constant effect over the whole range of variation. The error introduced by this assumption would be small in conditions where variations between individuals or time samples were normally distributed in a statistical sense, but the errors could become of the first order if the distribution of variation were skew or multimodal as is so often the case. One of the valuable functions of Cybernetics is to develop methods of studying complex interacting systems containing non-linear elements ; the principal assumption and fervent hope of Cybernetics is that the tactics developed for this purpose in one particular field may be illuminating in other problems as well.

The special difficulty of studying complex non-linear systems is most clearly seen in the case of the human brain, where the number of elements may be as great as ten thousand million and no experiment can ever be repeated. Each experiment on the intact brain inevitably modifies to some extent the internal organization of the system itself. The virtue and advantage of the Cybernetic approach is that it encourages the development of rigorous hypotheses which are sufficiently general to be tested in a variety of problems, yet specifically related to observed phenomena. Ideas relating to mentality in terms of brain function can easily be put into words, but semantic difficulties are immediately apparent. The terms in common use in psycho-physiological circles often have many meanings and the resulting ambiguities permit the theorist to change his ground under attack, without realizing that he is modifying his hypothesis so much that it may altogether change its character. The alternative expedient, a purely mathematical statement, is attractive but has the serious—though perhaps only temporary—disadvantage, that few experimental physiologists have enough mathematical training to understand and apply modern algebraic and topologic methods to their observations so as to design crucial experiments. It is evident that in the works of Wiener, McCulloch, Pitts, Rashevsky, Ashby, Mackay, Uttley and others, there are many basic hypotheses which could be tested by experiment, but it is still hard for the laboratory worker to realize how the expressions set up by these writers should be transferred into human situations or "hardware," and the purely mathematical essays have proved disappointingly sterile.

It really seems that the notions developed in such essays become clear and fertile only when they are embodied in working models such as the Homeostat and DAMS of Ashby, MacKay's SAM, and the learning and pattern recognizing machines of Uttley. Mathematics is a very sophisticated and unnatural language but a mathematical idea embodied in a model seems as natural and effective as a gesture.

The models just referred to were developed from general principles and in fact none of their creators is a brain physiologist. Approaching the problem from my starting point as an academic neurophysiologist I have been startled and comforted to find how my own fumbling attempts at producing "crystallized hypotheses" and these other machines seem to be converging toward the same concepts. To me, the most powerful notion, common to all these developments, is that the operation of what one may call intelligent mechanisms, whether in the flesh or in the metal, is statistically rather than logically determined. This would suggest that logical thinking is a special case of statistical reflection. An analogy may be seen with the evolution of ideas about material bodies and gases. The forces between two bodies are easily understood and computed, the three-body problem is almost intractable and above this degree of complexity there is a sudden transition to the statistical treatment of elastic gases and incompressible fluids. There is nothing in the Gas Laws or the Law of Fluid Motion that invalidates the molecular hypothesis, but the destiny of an individual is unimportant in the molecular population of a gas.

Thus in the study of the brain in relation to mentality, very valuable information about the activity of single cells is being collected at the same time as other investigators are studying, in effect, the cell-gas of the brain as a statistical assembly. Unfortunately for us physiologists the relations between nerve cells are less strictly limited than in the case of the gravitational or electrostatic forces between bodies. For one thing, the relation may or may not be reciprocal; in other words we have to accept in the brain the presence of many non-linear operators, as though astronomers had to allow for occasional one-way gravitational fields. This increases the intricacy of our problem in the following way. If we consider only two elements such as nerve cells, A and B, we can recognize the following situations :

(1) neither active

(2) A active alone
(3) B active alone
(4) A and B independently active
(5) A activating B
(6) B activating A
(7) A activating B and in turn activated by B.

Thus two elements have seven possible modes of behaviour even assuming all-or-none communication. If, as is more often the case in the nervous system, the elements are interconnected by a pulse-interval modulation system, other degrees of freedom must be added. Such a single binary system is easily embodied in a model, and endowed with rudimentary receptors and effectors. These models, which first saw the light seven years ago, exhibit the expected variety of behavioural patterns, particularly when the elements are allowed to form reflexive circuits and are associated with some form of " spontaneous " activity generator such as a noise source or a scanning element. They demonstrate in detail the general truth of the proposition that purposeful, discriminating and even " self-conscious " behaviour may be seen in quite simple systems that possess full reflexive freedom. Furthermore, when two or more such models are gathered together, rudimentary social systems appear with hints of co-operative and competitive features. Of interest also is the amplification of personal differences by interaction between internal organization and environmental incident, even in these simple automata.

This brief account of one of the courses suggested by the application of Cybernetics to the problem of Brain and Mentality illustrates immediately how easily and helpfully the definition of one problem may throw new light on others. In order to appreciate the analogy between the simplest reflex properties of the nervous system and the behaviour of synthetic mechanical reflexive automata, it was necessary almost at once to consider how the behaviour of individual reflexive systems is likely to be modified by aggregation of several individuals into some form of Society. Since the number of components in each individual was by design, minimal, and the complexities of individual behaviour were therefore easily predictable, the embellishment or constriction of behaviour introduced by social aggregation could be identified without ambiguity. One of the striking though elementary observations was that owing to the presence of non-linear operators at every stage (that is, in physiological terms, the fact that the receptors, central nervous system and effectors are all threshold devices) the intensity and variety of specifically social behavour is a very powerful function of *crowding*. In these creatures the main " positive reflexive behaviour " is related to the action of light. Even if there were no threshold the effect of one creature upon another would be at least an inverse function of the square of the distance between them. Since their behaviour is dependent upon mutual interaction, the effect of distance would be even more highly geared than in the unidirectional case, but the receiving organ is also *entirely* insensitive below a certain intensity of received light and this inevitably limits mutual interaction absolutely to a definite maximum distance between individuals.

The effect of this step-function of threshold can be very dramatic. If five or six 2-element reflexive individuals are released in a fairly large room and allowed to explore the whole of the space available, the chances of their meeting and forming even temporary unions are very small in the absence of positive stimuli in the form of light sources. If now the space available for exploration be progressively reduced by the approximation of obstacles to movement, a point will suddenly be reached where, as a result of these *negative* stimuli—the obstacles—the behaviour of each individual becomes closely and mutually linked with that of all the others.

It is important to emphasize again that before this stage the tendency to such aggregation is not merely very small but quite infinitesimal during finite time. This process of forced aggregation by restriction of freedom closely resembles the image of liquifaction of gases, in which, at a certain pressure and temperature, there is an abrupt change of state ; the previously compressible and diffusible gas becomes an incompressible and coherent liquid. The analogue of temperature in the case of forced social aggregation is quite properly the speed of movement of individuals since there is a good physical reason to suppose that the temperature of a gas is the indication of the average speed of motion of the molecules of which it is composed.

This inference that extreme restriction of movement would result abruptly in social aggregation or " liquefaction " could of course be drawn from a purely theoretical analysis of such a system, but the dramatic change in behavioural patterns would be hard to envisage in this way and still more difficult to compare with the effects of orientation towards a positive stimulus in the absence of constriction. The appearance of a common goal or incentive (in the case of the models the provision of a light-source), will tend to attract all the individuals separately as soon as they come within range of the light source, but until

they come so close to it as to interfere with one another's motion, they continue to behave as individuals. It is only when the concentration of individuals in the region of the positive stimulus or goal reaches a certain threshold that the features of competitive exclusion become evident, and may even result in the complete obstruction of the approaches to the goal, with consequent total failure of all individuals to attain it.

In the case of aggregation resulting from compression the tendency is rather towards the formation of co-operative assemblies ; even with these very simple models such co-operative organization has been seen to result in specific achievement. For example, if a gap is left in the enclosure constraining the movement of the individuals, the forced attraction between them has occasionally resulted in promotion to temporary leadership of the first individual to find by reflexive trial and error a way out of the enclosure. Furthermore, on occasion the leader of the successful escapade has sometimes been found particularly *insensitive* to the attraction of others, and it can easily be shown that the individual who is least likely to respond to the others of the community may be the one most likely to take the lead since others can respond to the defective individual whereas this latter cannot respond to them.

Mechanical hypotheses are as limited in sociology as elsewhere, but in a field so divided by controversy the exposition of a simple and easily demonstrable theory is permissible. Social conditions are an example of a complex system which is inaccessible to experiment as defined above, and it is peculiar that so few sociologists have taken the obvious course of building working models of their subject matter. Indeed the study of living human brains and of human society have this much at least in common, that they elude controlled experiment and lend themselves most readily to analogue computation. In the example given above it is perhaps inviting scorn to extend the analogy to direct comparison, but there is no lack, even in recent history, of examples demonstrating the effect of encirclement upon nationalistic enthusiasm and blind leadership. In the contrary case there are many present-day social systems in which both aggregation and competition arise from the quest for common satisfaction which is inaccessible if sought by all, but more attractive if attained by some.

An extension of this principle of the model hypothesis to the problem of long-term adaptation or learning has also given useful results. The convergence of the mathematical, physical and physiological disciplines is here most apparent, for consideration of learning by association as a statistical proposition helps to resolve many of the obscurities and contradictions which have hampered the evolution of practical psycho-physiology. The comparison between the Pavlovian experimental situation and " real-life " experiences is similar to that between binary transmission by an ideal noise-free channel and semantic signalling in a real noisy one. The Pavlovian animal is typically presented with unequivocal signals with unambiguous associations, whereas the free animal, and *a fortiori* the human being, faces a complex of contingencies in which no relationship is more than probable and great redundancy is essential. In the human case, " certainty " generally turns out to be a chance of more than about 10,000 to one in favour of one class of events leading to another. This approach to human learning opens up a fertile field of study, for many of the differences between individuals in character and taste seem to be based on differing estimates of the chances that are worth taking in a given situation. In brief, this view of mentality considers the brain as, among other things, a computer of contingencies, which, whatever the situation, is steadily estimating the chances in favour of an association between events being significant. Here again there is ample scope for individual variation since not only the criterion of significance but the way in which observations are sorted out and projected can vary over a wide range. Several mechanisms have been suggested as a basis for the learning process; in all of them the advantage of the cybernetic approach is evident, for they encourage analysis into clearly specified and experimentally identifiable operations instead of relying on vague and ambiguous vernacular phrases or massive intractable algebraic expressions.

An interesting question is whether in the operation of human society, there is any precise analogue to cerebral learning by association. An obvious answer is that the almost universal human passion for gambling is the social representation of this mechanism. Obviously, gambling is essentially and exclusively a social phenomenon. True, it is possible for the individual to gamble against himself or against the forces of nature, but it is extremely doubtful whether such an activity is deeply satisfying for more than a short time. There are few, if any, cases reported of people in solitary confinement or isolated by shipwreck passing their time in private games of chance. A few mathematicians in similar situations have amused themselves by tossing coins in

order to discover the distribution of runs and so forth, but these pastimes were devoted to a special end and are not claimed to provide either hilarity or excitement.

Social gambling is clearly an expression of what we may still, in small letters, describe as human nature; recently it has become a highly academic subject, generally considered in terms of the Theory of Games as in the treatise by von Neumann. There is almost no individual who has not at some time in his life felt an urge towards games of chance, and simple inspection of social trends suggests that the particular game chosen reflects very clearly both cultural patterns and individual traits. During the last generation the enormous growth of mechanical gambling systems such as the totalisator or pari mutuel in horse racing and the football pools competitions has extended big money betting to all classes. In assessing the importance of this phenomenon it should be borne in mind that the larger the scale of the organization, the less easy it is for a gambler to judge his chances of gain or loss and therefore the more likely he is to accept unfavourable terms.

In England the tax and profit deductions on football pool betting is such as approximately to halve the yield from a correct forecast, but this has no more deterrent effect than the widespread knowledge of the astronomical odds against a major gain. There is still a universal illusion that the chance of winning is a function in some way of skill or insight. It is obvious that the primary incentive in activities of this sort is related to the pleasure gained from pursuits governed by strict man-made rules ; a secondary attraction is the opportunity presented for cheating in some form. This pleasure may derive from a behaviour pattern so primitive and universal as to be almost worthy of the name " instinct "—the pleasure of elaborate construction followed by ruthless destruction, as when a child constructs a lofty tower from his playblocks in order to destroy it in an instant with his toy cannon. Students of children's play have suggested that this activity is characteristically masculine and is most frequently seen in phases of development marked by lack of security and jealousy of the father. Extension of this motion to social behaviour suggests that organized gambling has symbolic value as defiance of authority and the laws of nature.

It is quite simple to demonstrate such a process in the mechanical analogue of learning based on the computation of contingency. The model referred to above is usually adjusted so that a purely fortuitous association will occur only about once in a thousand trials under normal conditions. If, however, such an incident should occur, the instrument cannot discriminate between it and a truly significant association, and will store up information of fortuituous contingency in as great detail as any other. Information thus hoarded can be of no value except in relation to some other fortuituous occurrence which is unlikely to occur in a very simple system such as this particular model. In a more complex device, however, allowing for several thousand possible contingencies, there is a quite appreciable chance that any moment a number of fortuitous and " illusory " associations will be in the machine register. Such stored items are likely to have only one feature in common—that they are unverifiable. It is easy to see that from the stock of unverifiable associations there can easily be constructed a spurious system of myth and legend. This analogue therefore suggests that the prevalence of superstition and the power of the fairy tale should be directly related to the complexity of the system concerned and to the rapidity with which practical verification can be achieved. Here again, the application of this general principle to real human societies past, present and future is an easy and perhaps not unprofitable step.

In studies such as this it is difficult to see any argument against the use of analogue devices and it would be of great interest to construct such a model to represent some well known and thoroughly investigated human culture of the past and to see whether the model would operate so as to re-capitulate the evolution of the real culture up to the present day.

This feat has been attempted by classical algebraic methods but these, as already admitted, are almost impotent in conjunction with non-linear elements, and non-linear elements are of the essence in a system employing statistical selection, whether it be adapted to individual learning or to the establishment of a social tradition.

Needless to say, many of us in several countries are now hard at work searching in the brain for electric and chemical evidence of the functions which our models suggest should underly the processes of learning and thinking. It is too early to claim more than that the results are encouraging. We are still learning to design experiments and process our observations—learning also to work together in teams with many disciplines and diverse languages. This is the most serious problem before us ; to transform the theoretical

suggestions derived from cybernetic essays in analysis into practical experimental machinery and situations.

Nearly all scientific workers welcome an extension of their own subject even if it entails the destruction of traditional boundaries, but few of us were emotionally prepared and none of us were formally trained for the explosive widening of our domain which has come about during the last few years. As a physiologist I was content to assume that all individuals of the same species of animal were very much alike under similar conditions, and it came as a serious shock twenty-five years ago to find how idiosyncratic were the activities of the brain even in the crude terms of our early electro-encephalographic records. It was even more disturbing to realize recently that the social aspect of human behaviour was also reflected in the electrical characters of the brain. With present methods we find difficulty in considering the brain physiology of more than two people at once—we are stuck at the two-body problem—but one of the most exciting prospects for the future is the possibility of developing some way of studying larger populations in terms of the objective variations in brain function which accompany the interchange of ideas and affection.

A notorious weakness of economic and social theory is that in order to simplify and define the major problems, major variations between individuals must be almost completely ignored. In the more satisfactory approaches, variations are accepted but are assumed to be normally distributed. There is, however, no reason to suppose that variations of personal taste and inclination are distributed in a statistically normal fashion and even the most superficial consideration of social mechanisms suggests that some societies at least are excellent amplifiers of individual differences while others are equally effective attenuators of personality. This presents a very pretty problem for statement and analysis. Imagine a system, inherently stabilized by the usual reflexive processes, in which individual variation is *limited* by corrective social circuits ; suppose this system is then coupled in some way with another self-stable system which includes mechanisms for the *enhancement* and *exploitation* of individual variation. Now, what are the signs and values of the coupling co-efficients that will render such a binary assembly, stable or unstable ? This, of course, is a longwinded description of one of the conditions of present human society ; if we knew the answer, or even knew that there *is* an answer, most of our anxieties would be relieved or transmuted into action. In ignorance as profound as ours of the dynamics of such a system any action would almost certainly be wrong, and the present state of patient adaptation is the only one which Cyberneticians could confidently recommend, if analysis proves impossible. It seems a fantastic omission in the pattern of human culture, that there is not some co-ordinated effort to explore the possibility of such an analysis, since both the skill and the incentive are everywhere in abundance and the inherent interest of the problem, even on the theoretical plane, is very high. Recalling the patent similarities between this problem and those encountered in researches on living human brains, we might expect that, even if social cybernetics were to fail in extracting the roots of social violence, there might emerge—for example—a regime for the avoidance of epileptic seizures.

One last question can be considered—can we work out a cybernetic specification for identifying the essential components of living mechanisms?

Put in this way the question is too vague to provoke a serious answer, but I can give an example of the sort of problem that worries me. I have already mentioned that several of us have developed theories of learning and crystallized them in models, and I have suggested that, in general, the concept of learning is the most exciting and fertile field for cybernetic fertilization. Now, some of these models seem to me to suggest that there is something essentially " alive " in a mechanism which ensures that the more often an event occurs the more likely it is to occur again. But this seems to me to be perverse. In what way does such a mechanism differ from that of a torrent bed in which each storm wears a deeper channel, or a shoe which becomes more comfortable at each wearing ? Such processes seem to me to have nothing to do with learning and the construction of elaborate models to exhibit them suggests only that molehills can be made out of mountains. If we can decide that " improvement of performance," the better matching of internal organization and external conditions, is merely a special case of the general process of wearing down and running-in seen in so many non-living systems, then we can clear the ground for a more satisfactory specification of the adaptive mechanisms which are found only in living creatures and societies and in machines designed to discriminate between significant relations or pattern and random events. In the English language we make great use of this word pattern—it is one that causes great confusion and has no counterpart in many other languages. I suggest that a pattern is a relation between events which

will operate a learning mechanism. As a corollary, a learning mechanism will give no output when receiving a random input signal, so we may perhaps restate my question in such form as : what are the specifications for a transmission system such that a noise input gives zero output and the putput is a function of the improbability of occurrence of the input signals? I think we shall find that one solution is to consider the input as coming from two or more sources, and that if we feel committed to framing a cybernetic specification of living mechanisms we may have to consider not so much how goals are attained, but rather how it can be decided which goals are worth attaining.

Correspondence

M.K.S. units in physics teaching

I was very pleased to receive Mr. R. M. Sillito's support (in his letter in the May number) for my main contention that the m.k.s. system should not be used in schools. It is also heartening to see that he shares my low opinion of some of the teaching methods proposed for school use, though he falls a victim to Goodier's assertion that "there is no way of measuring H save with an ammeter and a metre ruler." Gauss measured H (not B !) with his deflection and vibration magnetometers before ammeters were invented.

I am sorry my article seemed to Mr. Sillito a "condemnation of the m.k.s. system." I was careful to point out in the first paragraph that my criticisms were not of the system but of its use in elementary physics teaching ; and, re-reading the article, I do not think I wrote anything against the system itself, which I personally find very convenient for most purposes—except elementary teaching.

K. A. Macfadyen (*Fellow*).

University of
Birmingham

Traditionally we teach electrostatics and magnetostatics as if they were quite unconnected and we derive a system of units from each. Not until later do we discover (or admit) that the two systems are related by the velocity of electromagnetic waves. It should surely be possible to devise a teaching sequence that is just as logical as the old one but that starts at one point not two.

The universities largely control what is taught in schools, by means of examination syllabuses. I hope the universities will bear in mind that a school course must not only be a firm foundation for a university course, but must be complete, interesting and satisfying to pupils who leave school at 16 or 18. Electrostatics and, to a lesser extent, magnetostatics are the parts of the syllabus which we could best afford to jettison. Perhaps if we were less preoccupied with the formula for Gauss position B, Mr. Weintroub would meet fewer students who had never heard of Rutherford.

Mr. Macfadyen could hardly have chosen a worse analogy to illustrate his contention that B and H are of the same nature and should be measured in the same units. Mass and weight are most certainly not of the same nature and yet sometimes the same unit is used for both. Confusion is made worse because the process of finding the mass of a body on a chemical balance is called "weighing" and the standard masses used are called "weights." These terms are so well established that I doubt if they could be altered, but the Physics G.C.E. syllabus could well do without gravitational units and confine itself to absolute units.

When I studied electrical engineering I learnt B = flux density, H = magnetizing force, $B = \mu H$, almost as a cause and effect relationship. Later I studied physics and had to learn B = magnetic induction, H = field strength, $B = H + 4\pi I$ and I found it all very confusing. Whether electrical engineering seemed easier because I met it first or because the lecturer was better I know not.

(Mrs.) H. R. Taylor (*Associate*).

Greycot,
Strines,
Stockport,
Cheshire.

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Presenting Science in Physics Textbooks*

By B. C. BROOKES, M.A.

(*University College, London*)

INTRODUCTION

When we consider how science is presented in physics textbooks, several kinds of questions arise for discussion :—

What are physics textbooks for ?

Do we get the textbooks we need ?

What are the responsibilities of the authors of physics textbooks to their subject and to science in general ?

In discussing these questions I shall have in mind those which we use in schools, in technical colleges and universities in courses leading to first degrees. I am deliberately excluding from consideration the specialist monographs written *by* and *for* advanced workers. I am concerned with teaching only.

To clarify our views about the adequacy of physics textbooks we shall have first to consider some rather fundamental attitudes and theories. We shall have to examine our epistemologies, i.e. the views we hold about knowledge and about science. One's belief in what constitutes a good physics textbook will depend in part on :—

(i) The " theory of presentation " that one holds ; for example, how one regards and describes the process that goes on when a student *reads* a physics textbook.

(ii) The " theory " that one holds about science in general, and about physics in particular, as subjects of study and about the purpose of studying them.

Since both of these theories are only rarely aired or rationally discussed, and since they form part of every man's deep-seated fundamental attitudes, based on his life-time of experience, they are difficult subjects to dig up and to display adequately. But it is important that we should try to examine them, because I believe that most people are fundamentally in error in their attitude to presentation and to knowledge, and that their errors are the cause of some of our present troubles. Pre-eminent among these errors is the belief in the dichotomy that puts scientists in one camp and humanists in another—a spurious, futile, and damaging dichotomy that scientists too readily accept as real. Unfortunately, scientists give support to those who believe in this dichotomy by the kind of textbooks they too often write. At the same time, these textbooks only repel many young people who would enjoy and contribute to the activity we call science if science were more intelligently presented to them—the intelligent young people we need. Let me explain.

THEORIES OF PRESENTATION

I have spoken of the " theory of presentation " that one holds. Some of you are perhaps a little surprised to find that I assume that you have such a theory. Yet I think you must have, even though you may never have tried to make it explicit. Since you are all physicists you must at some time have written reports on your experimental work, or papers expounding some development of theory, if not a textbook. If you write, or have written in this deliberate way, then you have a theory about written presentation. There must have been occasions in which you first drafted a report, a paper, a chapter of a textbook, and then corrected your draft version—amending it by deletions, additions, rearrangements, changes of emphasis, and so on. Even if you were satisfied with your first draft, you must have used *some* judgement, *some* selection, perhaps not very conscious or deliberate, in your writing. You wrote what you actually did write rather than something else. All this implies that you have some criteria by which you select, compose, and amend the words you write. You might be hard-pressed if you were asked to explain to someone else *why* you chose to write in one way rather than another, *why* you amended such and such a phrase to something different. These criteria, implicit though they may be, constitute what I call your " theory of presentation."

It is *your* theory of presentation, whatever it may be, that each of you individually applies, not only to your own writing, but to the writings of others, and so to your critical assessments of physics textbooks. My main difficulty is that though I can assume that we would be fundamentally agreed about any physical theory we might discuss, I cannot assume that we are fundamentally agreed about our theories of presentation. My experience suggests that immediate agreement is unlikely. Any differences that emerge may appear at first sight to be trivial, but the logical

*A lecture given to the Education Group of the Institute in London on January 22, 1958.

or conceptual structures that grow out of apparently trivial differences in their basic premises can lead to marked differences in conclusions, in behaviour, and to unresolvable disputes when, with the original differences lying undiscerned, the differing conclusions are applied to practical affairs (*vide* politics). Therefore I will try to make explicit the theories that are commonly held, because to recognize them and their limitations *is* crucial in any discussion of the problems we are concerned with.

The Platonic Theory

When I press students and others to formulate their theories of presentation, the answers I get vary in minor ways, but I find that they mainly fall into one of two classes. The most widely held of the theories is expressed in Platonic terms such as "ideas," "mind," "thought," and so on. Those who hold it speak of "conveying ideas from mind to mind," of "expressing their thoughts in words." The process of reading a textbook would be described, in its simplest form, in some such way as this : "The author of the textbook has ideas about physics which he expresses in the words that are printed in the textbook. As the reader scans these words, his eye sends messages to his brain, where, in some way unknown, the words are stripped from the expression of the idea ; and thus the idea that began in the mind of the author is conveyed or communicated to the mind of the reader." That is a summary of the most generally accepted theory and the way in which it is most commonly described. Some reply, however, in the more sophisticated terminology of information theory—"idea" is translated into "information," "words" become "signals," "convey" or "communicate" become "transmit" and "receive." In this terminology the process of reading a text-book is described somewhat as follows :—"The author of the textbook encodes his information in the words and other symbols printed in the book and thus transmits his information to the reader. The reader receives the transmitted signals, decodes them, and thus receives the information the author wished to transmit."

In spite of the different terminologies, the two versions are essentially the same. Both regard "knowledge," "ideas," "information" as something that can be contained, wrapped up in discrete parcels and despatched from one man to another rather as the grocer receives and retails his crates of soap. Though this theory is widely held, though it may be expressed in more sophisticated ways, though it has the most reputable origin, though it has been uncritically accepted for so long that it permeates our language and way of thinking, I think it is fundamentally and mischievously wrong. At best it can be regarded only as a highly metaphorical description applicable to a few limiting cases. There is no such thing as "an idea," or "information," or "knowledge" which can be trasmitted in any literal physical sense from mind to mind. And yet, even if we are aware of its metaphors, we may still be led to believe in and to act on the implications of the theory, even to deny the theory while acting in conformity with it. Those who hold this theory are sometimes more easily recognized by what they do than by what they say.

The Platonist teacher looks upon himself as one with a store of ideas to impart and he believes that the imparting of these ideas can be achieved simply by standing in front of his students, expressing his ideas in reasonably well constructed and audible statements. His ideas are thus conveyed from his mind to the minds of the students before him—and any failure of this process he attributes to the inattention or intellectual deficiencies of his students, not to his theory of presentation. The Platonist teacher also draws up exhaustive and minutely detailed syllabuses for his subject and then ticks off the items one by one as he expounds them. He is keen on marks and examinations.

The Platonist textbook writer is by inclination encyclopaedic. It is he who produces the compendious, comprehensive, all-in-one treatises of physics, neatly subdivided into Heat, Light, Sound, etc.—the whole of basic physics neatly wrapped up in several hundred pages and delivered to the consumer in one bound volume. In his preface the author will claim "to have covered the syllabuses" of the Intermediate, the Scholarship, and other specified examinations.

This, then, is how I should describe what I mean by the Platonic theory of presentation and by what deeds, if not by words, we may recognize those who hold it. The theory is fundamentally false ; it leads to bad teaching and to dull textbooks. It is a theory widely held in academic circles and will take time to displace.

The Aristotelian Theory

The second of the commonly held theories of presentation—though not as popular as the Platonic theory—is expressed in Aristotelian terminology. Aristotle was more of a scientist and logician than his teacher, Plato, and those who hold the Aristotelian theory are, I find, usually mathematicians or mathematical physicists. They describe their theory using the words "symbols," "meaning," "referent," or "thing referred to,"

They would say, for example, that the word "spectroscope" refers to a certain optical instrument. The instrument itself is thus the meaning of the word "spectroscope." Similarly, they go on (though only when pressed) to say that the words of a physics textbook refer sequentially to a number of things such as spectroscopes and so the reader who is familiar with the things the words refer to will understand the text. At first sight this theory looks convincing, but it too runs into difficulties. This too is applicable only in a limiting case—that in which the words are concrete objects which can be seen, handled, pointed to, or otherwise physically brought to the attention of the reader or listener, though the symbols and statements of logic and pure mathemaics may also be accounted for in this way. It is more difficult to find the "referent," the thing referred to, if the noun is, for example, abstract—e.g. honesty, truth, matter, velocity—and even more difficult if the word is not a noun but one of those logical constants that we use so often—"and," "if," "or." Neither an English sentence, nor *a fortiori*, a physics textbook, is a sequence of symbols each with its own discrete referent yielding its "meaning" as an algebraic summation of the meanings of the separate words.

Modern exponents of this theory have therefore been driven to rather desperate measures to "save the phenomenon." One way is to argue that the word does *not*, after all, refer to the thing itself, e.g. to the spectroscope itself, but to the *concept* of the thing, e.g. to the concept of the spectroscope. But the concept of a spectroscope is a wholly private affair which cannot be reached by any outside observer except possibly through its expression in more words. And so this attempt to varnish over the cracks in the Aristotelian theory leads to a dilemma. One has to decide either to pursue an infinite regress of words, concepts, words, concepts . . . or to admit that words are symbols which stand for something though, in general, we do not know what.

So this theory, too, is at best a metaphorical description applicable only to a limiting case. It is a very dubious foundation on which to criticize any kind of writing or expression—scientific or otherwise.

The Limiting Cases

The limiting cases in which both theories give an adequate description, adequate for most purposes, of the writing-reading process is that in which all concerned are agreed about the exact context in which the words apply and are familiar with the usages, in that context, of the words employed. For example, the author of a physics monograph is in the happy position of knowing with fair certainty that his intended readers use the language and symbols of physics as he does. In such a case we can say with some precision that "ideas are being conveyed from mind to mind," from the mind of the author to the minds of his readers. We can say that his "concept" of "spectroscope," "nucleus," "dipole" . . . , his "meaning" of these words, is close to the concepts or meanings these words have for his specialist readers. But this case in which both theories apply as metaphorical description of what occurs is, from a general point of view, a relatively rare occurrence. From higher academic points of view, however, it is a relatively *frequent* occurrence, and this fact may account for the tenacity with which these theories are held in academic circles.

When we consider the writers and readers of textbooks, however, we can no longer assume that they are agreed about the contexts, the meanings, the concepts, the ideas, the usages of the words or about anything else. Yet all to often it seems to me, the textbook author writes as though this agreement had been achieved or could be taken for granted. He assumes that his ideas can be conveyed to his young readers by writing his textbook as though he were writing an advanced monograph, except for some restrictions on the technical terms he uses.

Context Theory

You may well ask what I propose to put in the place of these theories that I have condemned. I have to confess that there is very little one can at present say about this complex process of presentation. In recent years there has been a trend in philosophy away from Platonism and from the linguistic analysis of statements that follow from it. Philosophers less frequently take sentences out of their contexts, hold them up to the light, and ask themselves what these isolated statements mean. They have found that isolated statements can mean anything or nothing at the whim of those who examine them.

Wittgenstein has called this process "examining language that is 'on holiday'." Nowadays some of them at least, begin to ask, before they consider the meaning of a statement, where did this statement occur ? In what context was it altered? This is an important step in the right direction, but the philosophers have not yet gone quite far enough. It seems that when philosophers consider the context of an utterance they still imagine it to be peopled by human beings of infinite wisdom and maturity, by people who are completely grown up. But the world of everyday life, and particularly the world in which physics

textbooks are used, is peopled by young and growing human beings—as well as by their teachers.

When we ask the meaning of a statement we must first know in what context it was uttered. A statement that is medical and scientific in one context may be obscene in another ; one that is religious in one context may be blasphemous in another. But we must also consider *by* whom and *to* whom the statement was made. If *I* tell you that the Bank Rate is going up to 10 per cent you are unlikely to be alarmed, but if the Chancellor of the Exchequer made the same statement you would take him seriously and possibly change your plans about your mortgages and overdrafts. So all we can say about this complicated process of writing and reading textbooks is something like this : " The author of the textbook, knowledgeable about physics, presents or displays a certain sequence of English words and physics symbols in his textbook ; his student reader attends to this display, and interprets the display in the light of his experience."

Even that statement or description is vague and metaphorical but to say more is to attempt to cover our ignorance of the complex mental processes we call writing and reading under a blanket of high-sounding verbiage. The statement has at least the merit of implicitly admitting our ignorance. And so I try to avoid the words of the Platonic description—ideas, thoughts, mind, communication—and the words of the Aristotelian description—symbol, meaning, concept—in talking critically about presentation because they give the user the strong but misleading conviction that when he uses them he is saying something useful.

Nothing is communicated : there is only something *displayed* and this display is *interpreted* by anyone who attends to the display.

Some of you may realize that I am taking a nominalistic view of language and a behaviouristic view of human activity. I admit it. Our next step is to consider what science, and especially physics, looks like when seen from this point of view.

First let us consider what we are trying to do when we teach physics. Following Wittgenstein I would say that we are trying to teach our students a special language game—how to use the vocabulary and syntax of the language or dialect of physics. But this is not to suggest that physics or any other science is purely a verbal activity. Science is a practical activity closely related to the events that occur in our physical environment; the language we use in science is not the whole of it but is an essential part of it. Certainly without the language there would be no science.

I have to emphasize the very close relation between the language and the practical activities in which the physicist engages, otherwise we should find ourselves back in the verbalism of those who hold Platonic theories of presentation. The language and the activity—the activity of experiment and observation—grow one out of the other, each acting as a check and control on the other. It is futile to separate them.

When we talk together as physicists we are using a special language or dialect, or playing a language game, that has slowly been forged and established for us by our predecessors over the past 300 years. The main characteristics of this dialect are :—

1. We have great confidence in it because we find that it works, it is effective. We find that it enables us to write and talk in a way which others who know the dialect find to be clear and unambiguous. Perhaps more important, it enables us to modify our physical environment. Its more spectacular manifestations — atomic bombs and sputniks—are obvious to everyone.

2. It is a living, developing dialect and has built into it the means of further development. For example, words like " atom," " mass," " radiation " have uses different from those of 50 or 60 years ago. It is this possibility of development that makes physics intellectually attractive. In the research journals we find authors straining and testing the dialect as they apply it to new activities and inventing new counters in the word game to play with.

3. It is an international language which can be acquired whatever the natural language of the student may be. The Chinaman, the Russian, the American or Englishman talking physics use words and symbols in the same way. As far as it is necessary to do so, it is always possible to translate physical statements from English into Russian, or any other natural language, without ambiguity or intellectual difficulty. Physicists, whatever their natural language, are intelligible to each other because they are fundamentally agreed. Disputes or arguments which arise are not purely verbal ; they can, at least in principle, be settled by recourse to the activity of observation and experiment.

4. It is self-correcting. This, of course, is the crux of physics—or any other science regarded as a dialect. The physicist who reads a physical statement that he doubts can always, again in principle, test it. And test it, not merely by linguistic analysis, i.e. by consulting a dictionary or by studying the way in which other people

use the words of the statement. He can test the statement first by having recourse to logic because the statement must conform with the syntactical rules already established in physics and formulated in its laws and principles, and, secondly, by making relevant observations. For example, the word "force" is defined for physicists as Newton defined it—as rate of change of momentum—and this definition has long been accepted. Any deviation from it, without the consent of physicists, would be regarded as an offence against the syntactical rules of the physics dialect—and therefore as bad physics. As a second example, one might read that some new metallic alloy was said to have a negative coefficient of expansion over a certain temperature range. Such a statement cannot be dismissed as offending against the syntactical rules of physics because there is no fundamental law which says that all substances expand when heated. But the likelihood of a substance contracting on being heated is small—we call such an occurrence in water "anomalous" and so we might well doubt the statement. But the verification of the statement in this example can be made only by acquiring some of the alloy and measuring its change of dimension over the given temperature range.

It is in this possibility of verification by logical or experimental activity—where verification includes always, as Popper has emphasized, the possibility of rejection—that the success of the dialect of physics lies.

5. The last important characteristic of the dialect of physics, implicit in its effectiveness, but which deserves particular emphasis, is its excellence as a means of "communication." From one point of view, the dialect of physics can be regarded as an attempt to evolve an ideal Platonic language (there may be others) in which those who understand it can speak with precision and without risk of ambiguity. It falls short of the Platonic ideal only insofar as it depends (as it must in part) on the natural languages in which it lies embedded. The natural languages are full of imprecisions and vaguenesses, however adequate they may be for the casual affairs of everyday life. The linguistic aim of the physicist is to purge his dialect of all subjectivity and vagueness—an aim which, though unattainable, gives direction to his efforts.

Other kinds of scientists, notably the biologist J. H. Woodger, are aware of the imprecisions and ambiguities of their own scientific dialects. In recent years Woodger has tried to establish a purified dialect, a symbolic notation which will serve the needs of biological description and discussion. But, in spite of its demonstrable advantages, his symbolic system is probably premature. Biologists have not yet reached sufficient agreement about their very complex subject and the way in which it should be discussed. Whatever advantages the symbolic system of Woodger, or of anyone else, may have, the forging of a new dialect depends not on the proposals of one man ; it is a slow social process depending on an implicit acceptance by those who need to use it.

The physicist is fortunate. He was first in the scientific field. His dialect, and that of the chemist, have become models for all who wish to look at their environment in a detached objective way. But that is no reason why the physicist, having forged a satisfactory dialect, should forget his great dependence on it. I am suggesting that physicists—especially *teachers* of physics—should be more conscious of their dialect, proud of its achievements, but aware of its limitations. Such an awareness would help them in their teaching, make physics more attractive to those young people who are at present repelled by it, and help physicists to write better textbooks.

So far I have tried to describe those different fundamental attitudes to presentation and the different practical consequences of holding these attitudes. I will now summarize them to show as clearly as I can the implications of these three attitudes for teaching, for learning, and for physics textbooks.

1. *Platonic*

(*a*) That physics consists of a heap—a growing heap—of knowledge which can be expressed as "atomic facts."

(*b*) That the task of the teacher and of the textbook author is to express these atomic facts—neatly segregated into sub-heaps—in the form of clearly audible or clearly readable statements.

(*c*) That the task of the student is to absorb the piles of atomic facts presented to him.

(*d*) That the student's absorption of these facts can be tested by examination rather as one can measure the depth of liquid in a tank by pushing a dip-stick into it.

2. *Aristotelian*

(*a*) That underlying what we see or write or do in physics there is a firm unchanging reality that can be reached if only we strive hard enough to observe it, and that this firm reality can be unambiguously expressed or described in symbols and by the logical relations between them. One of our great mathematical physicists, Clerk Maxwell, put it this

way : " Physical science is that department of knowledge which relates to the order of nature."

(*b*) That the task of the teacher or of the textbook writer is to express this reality as clearly as possible—to put into words this one true meaning.

(*c*) That the task of the student is to discover with as much certainty as he can the one true meaning of the symbols he reads.

Context—Behaviour Theory

(*a*) That physics is one form of social activity—a social skill—not, as Dingle states it, " an activity of *the mind* "—in which we try to correlate our experience with the experience of others.

(*b*) That the task of the student is to make his own discoveries (i.e. learning is *discovery*) and to correlate them both with his own experience and with the experience of others.

(*c*) That the task of the teacher and the writer of textbooks is to provide the student with opportunities for making his own discoveries and for exercising this social skill we call physics.

(*d*) That there is no such thing as " knowledge "—there are only individuals who know; that there is no such thing as " science " —there are only individuals engaged in scientific activity.

PHYSICS TEXTBOOKS

Remembering these three differing attitudes, let us now consider our present needs. What kind of textbooks have we got ? What is wrong with them ? What kind ought we to encourage ? From now on I shall give my personal opinion as one who holds that the last of the three theories of presentation provides the best criteria for criticism.

To answer these questions I went to the shelves in my college physics library to read the prefaces of the textbooks I remember from my student days, of the textbooks that have become popular in the intervening years, and of the famous textbooks of an earlier age, now mainly of historical interest. I selected books at random, moving from shelf to shelf as eager students made their own selections. I wondered what they were looking for. Were they seeking knowledge ? looking for ideas? Were they seeking the one true meaning? or trying to clarify their concepts ? Or were they trying to make discoveries and to improve their social skills ? I wanted to ask them, but doubted whether they would have understood my questions or that I would have liked their answers.

So I went on reading the prefaces. Of course I realize that prefaces have to be taken with a grain of salt ; the high-flown aspirations of the author's preface are not always realized in the subsequent pages. But nevertheless the preface gives a clue to the author's attitudes to physics and his approach to his job. Here is one :—

" My desire and aim in writing and re-editing this book were, and are, to arouse interest as well as to impart instruction. Labour is not to be shirked in the study of a great question ; but it may be lightened in two ways ; first, by the diminution of its absolute amount ; and, secondly, by calling forth an energy which shall diminish it relatively. The true teacher, with the discipline of his pupil in view, will, I apprehend, always invoke the positive in preference to the negative force. . . .

" Throughout the book I have endeavoured to pare away what could be spared, and to add what on reflection seemed worthy of introduction, but which had been previously left out.

" Not the results alone of scientific inquiry, but the operations of the inquiring mind, are of interest to the reader here in view. I have therefore tried to show the tendency displayed throughout history, by the most profound investigators, to pass from the world of the senses to a world where vision becomes spiritual, where principles are elaborated, and from which the explorer emerges with conceptions and conclusions, to be approved or rejected according as they coincide, or refuse to coincide, with sensible things. By his observations and reflections in the domain of fact the scientific philosopher is led irresistibly into the domain of theory, his final repose depending on the establishment of absolute harmony between both domains. Thus the motions of the solar-system rest securely upon the Principle of Gravitation ; light reposes on the Theory of Undulation, while it is the object of this book to show that thermal phenomena find a similar basis in the Mechanical Theory of Heat.

" On the Continent, science leans on the strong arm of the State ; in England its advancement must depend upon the sympathy of the public. Hence the supreme importance, in our case, of spreading abroad correct notions regarding its capacities, achievements, and aims. The practical triumphs of our day are obvious enough, and they are still frequently spoken of as if they constituted the entire claim of science to the world's attention. To some it seems a kind of handicraft, while others think it is, or ought to be, a mere congeries of facts. But they who regard it thus can know but little of the logic which runs through,

and binds together, that " System of Nature " which it is at once the glory and the responsibility of science to investigate and unfold. Far be it from me to claim for science a position which would exclude other forms of culture. A distinguished friend of mine may count on an ally in the scientific ranks when he opposes, on behalf of literature, every attempt to render science the intellectural all in all. Ours would be a grey world if illuminated solely by the dry light of the understanding. It needs equally the glow and guidance of high feeling and right thinking in other spheres. But this may be conceded while affirming the just and irrefragable claim of science to a more liberal space in public education than that which it is now permitted to occupy."

J. TYNDALL, *Heat as a Mode of Motion* (1863).

I seem to have heard that cry before ! But I wholly approve of his approach. It was a famous book and Tyndall a famous teacher.

Now let us turn to a more modern work. I quote : " In this book an endeavour has been made to cover the syllabuses required in physics for the Intermediate, the Higher School Certificate and Scholarship Examinations of the various universities."

That tells me all I want to know. I think you can imagine the kind of book it comes from—there are, I think, too many of them about at the present time. They are usually large, heavy, meaty, stuffed full of facts. It is clear that I do not like them. Yet I have to admit their popularity both with teachers and students—with teachers because teachers of physics are scarce and overworked, and because such all-in-one textbooks save them time and trouble (" Read Chapter 9 and do exercises 1-5.")—with students because again it saves them thought and trouble (" Mop up this lot and I'm O.K. for Inter ")—they have been forced to take a Platonic view of physics and of knowledge and of learning—the examination system imposes it on them. I have to admit that even with this system some students make good physicists. There are at least three possible reasons for this :—

1. The normal young human is tough enough and intelligent enough to survive almost any educational system thrust upon him.

2. The textbook he is asked to read is only a part of the student's teaching. A good teacher can provide the incentives, the interest, the intellectual adventure, that a dull text book fails to offer. And practical work in the laboratory—I think universally admitted to be an essential part of all science teaching—can help to bring alive the dead words of the dull textbook, and, if suitably designed, can offer the student his opportunities for making his own discoveries and developing his practical and social skills. So the textbook, good or bad, is only *part* of the story.

3. There may be a few students who are actively attracted towards the study of physics who will stomach any kind of dull work that will help them to some inner vision they have, probably of research. It is this inner vision that helps them to co-ordinate the bits and pieces of the subject they pick up. But there are many without this inner vision.

There is much to be said, however, for discouraging encyclopaedic books of this kind. My main reason is that they suggest to the student that physics is a solidified, petrified, crystallized corpus of knowledge and that the massive textbook is its authoritative expression. It suggests a permanence, a solidity, an authority, almost a dogma that is wholly alien to what physics or any other science should appear to be. It presents physics as a rounded, completed monument to the past rather than as a restless, endless, inquiry into the future. By so doing I think it repels many of our more intelligent schoolboys and schoolgirls. From my discussions with university students about the subjects they have chosen to specialize in, I am led to believe that they are not so much attracted by the subject they finally choose to study as repelled by all the possible alternatives. Physics is not the only subject that is not, in general, as well taught as it might be. And so, it seems to me, that textbooks of physics should suggest by their appearance as well as by their content, that the records of science are to be regarded as ephemeral, expendable papers to be discarded and to be superseded by something new and better as soon as possible. At all costs they should suggest change and progress and intellectual activity rather than permanence and stability and finality.

For that reason, incidentally, I noted with pleasure the slim volumes of the Institute of Physics' Monographs for Students. I hope they are succeeding as well as they deserve to. Textbooks *can* be important in attracting students to their subject. I shall always remember my astonishment, twenty years ago, at seeing a large dormitory of 14- and 15-year schoolboys reading in bed—simply and solely for pleasure—a new and attractive chemistry textbook. I would like to see that happen again—with a physics book.

My last major point is that though these large comprehensive textbooks may be erudite, well-written, and thoughtful selections from the field of physics, their weight, size, popularity and authority impose a rigidity on the teaching of

physics. There is a perpetual need to re-present the subject anew for each generation of students. There has to be a steady discarding and re-valuing of material and methods that can be superseded by more compact, more comprehensive and more up-to-date versions. I doubt whether this revision takes place quickly enough, whether it goes far enough, and whether the hysteresis is not increased by the large textbooks I complain of.

CONCLUSION

You may have expected me to deal with the problem of presenting proofs of formulae, descriptions of apparatus, intractions for performing laboratory work, and so on—at what I would call the superficial textual level. In general there is very little that can be said in criticism of these details. On the whole, physics books, or at least those that achieve publication, are adequately if not well written. My point is that you cannot even criticize this superficial level without reaching agreement on the more fundamental aims and purposes of your work. Unless these deeper aims are agreed there will only be unresolvable disputes about the more superficial aspects of textbooks and teaching. It is much more important to have the right kind of textbook, even if a little crudely written, than to have the wrong kind of textbook, however beautifully presented it may be. The physicist author's first duty is to physics rather than to the English language.

I have tried to show you my own attitude to teaching. In physics, I think it is not enough simply to stuff your students with facts, hoping that in the course of time many will put the atoms together for themselves and see physics as a unity. Our job as teachers is to provide our students with the " opportunities for making their own discoveries " and to show them that physics is an activity, not a process of passive absorption. Moreover, we have to show that it is both an individual and a social activity. For the individual it is an intellectual adventure, imaginative and creative, but restrained and disciplined by the need to communicate his work to others and to submit to their criticism ; this is the point at which physics becomes a social activity. This social aspect of physics and of science is too often overlooked ; I have tried to emphasize the need for teachers to be more aware of it—hence the time I spent talking of the dialect of physics—the means of communication between physicists. If we neglect this aspect of physics we have technicians rather than scientists, mechanics rather than engineers. Science is a public and social, not a private activity—and the scientist must be able to talk and write about his job in terms intelligible to other scientists, otherwise he is being a technician or a craftsman. You must try to interest the student physicist in his own dialect.

If my theories about presentation are acceptable, I shall be very surprised and rather disappointed if I find that I have in fact conveyed my ideas from my mind to your minds, that I have made my meanings and concepts clear to you, or if I have suggested that what I have written is " the truth, the whole truth and nothing but the truth."

Reviews of Books

FEEDBACK THEORY AND ITS APPLICATIONS

By P. H. Hammond

London : English Universities Press Ltd. Pp. 348. Price 35s.

This work is intended for postgraduate engineering and physics students but there is little doubt that many parts of it are suitable for undergraduate engineers at universities. The book could have been made even better for the latter purpose if it had included a lengthier treatment of mechanical systems and analogues, although a speed governor is treated at the very beginning of the book as an example of a non-linear element and incidental mention is made here and there of other mechanical analogues (on page 190, for instance, where a mechanical transitional lead system is treated). In this connection also chapter 13 on electronic simulation of control systems is especially valuable. This relatively trivial criticism should not be allowed to detract from the book's value.

The diagrams are unusually well drawn and the literary style is clear and concise.

There are detailed chapters on basic concepts and notation, the feedback principle, stability of linear feedback structures and graphical methods of representing the frequency response function. Chapters follow on various valve amplifier circuits (cathode followers, etc.) and on virtual earth amplifiers. Servo mechanism are then treated in another group of chapters in which many matters such as the frequency response, non-linearities, etc., are fully discussed. Finally, there is the above-mentioned chapter on electronic simulators.

J. D. Craggs.

NUMERICAL ANALYSIS

By K. Kunz

London : McGraw-Hill Publishing Co. Ltd. Pp. xv + 381. Price 60s.

This book is based on a series of lectures in numerical analysis delivered by the author at the Computation Laboratory of Harvard University during the year 1947-49. Acknowledgement is made to Professor Howard Aiken of that laboratory for his influence in the early stages of preparation and the book is of the high standard we should expect in such circumstances. The author's aim is to develop a fundamental understanding of the use of finite-difference methods in obtaining numerical solutions of problems in applied mathematics. To this end, some of the early chapters deal in some detail with finite-difference notation, interpolation and numerical differentiation and integration. These form the foundation of subsequent chapters on the numerical solution of differential equations, both ordinary and partial, and on integral equations. Several methods of solving a set of simultaneous linear algebraic equations are discussed in another chapter. The determination of the real roots of an equation is dealt with in the first chapter and the second is reserved for methods appropriate only to polynomial equations. Indeed, most of the chapter headings familiar in books on numerical analysis are to be found here though there is deliberately no mention of harmonic analysis, least-squares approximations or the smoothing of data. Some methods are published for the first time in book form. Of particular interest is the author's own way of designating the many quadrature formulae and of obtaining them, and interpolation formulae, from a lozenge diagram. There are numerous examples for the reader to attempt at the end of each chapter.

Throughout the book, attention is given to error terms and to the estimation of the accuracy of solutions. The author has tried to concentrate on methods particularly suited to electronic digital computers. The selection of such methods is, however, a risky business in these days of rapid development and the author wisely includes some methods which were not, at the time of writing, in vogue, but which make useful contributions to the students' general background of knowledge. In fact, one attractive feature of the book is that it contains such a large number of methods of carrying out various processes.

There are, on the other hand, curious omissions, some, but not all, of which can probably be attributed to inadequate revision of the ten-years old lecture material. Thus, there is no mention of implicit formulae in the treatment of partial differential equations of the parabolic type. The treatment of characteristics is relatively scanty. Central-difference formulae receive little or no mention in connection with the solution of differential equations. The troubles associated with ill-conditioned sets of simultaneous equations are also passed by, and there is only a brief reference to the determination of latent roots. There is no mention of the H.C.F. process for locating repeated roots. However, it is unreasonable to expect

more in a book which already contains so much and is so lucidly written. Rather should the author be congratulated on having completed so formidable a task so successfully.

J. CRANK.

CONCISE GUIDE TO PLASTICS

By F. R. Simonds

London : Chapman & Hall Ltd. Pp. xi + 318. Price 36s.

This is essentially a book on the American plastics industry and gives brief, but useful, notes on a wide range of the current plastics materials. It is designed to assist persons wishing to estimate the value of plastics in their own particular work and should be of value to Americans. However, its exclusive preoccupation with the U.S. restricts its usefulness to others. This will be realized when it is appreciated that all the trade-names are American. It should assist those engaged in the British plastics industry, partly because of those very trade-names, partly because of its lists of prices (although, as the author realizes, these are only a guide) and also because it devotes 44 pages to brief accounts of leading U.S. manufacturers and their plastics products.

The presentation of facts on the materials and their properties is quite good, although the method of dividing the subject-matter has led to considerable duplication. This is inevitable and not disadvantageous in a book designed to serve readers who may wish to commence study of the products of the plastics industry from different points of approach.

One cannot recommend this book for the general British reader. It should be of value to the plastics specialist as a complement to the comprehensive *Fibres, Plastics and Rubbers* by W. F. Roff (reviewed in the *British Journal of Applied Physics*, **8**, 11, p. 465.)

H. A. NANCARROW.

THE PHYSICIST'S CONCEPTION OF NATURE

By W. Heisenberg. Translated by Arnold J. Pomerans

London: Hutchinson and Co. (Publishers) Ltd. Pp. 192. Price 16s.

This book, by the author of the principle of indeterminacy, falls into three parts. In the first, which occupies some 50 pages, Professor Heisenberg discusses the problem of nature in the light of modern physics. This is the section which his own outstanding contributions must invest with the greatest interest. There is little in it that will be new or unexpected to physicists. He stresses the change in the physicist's object of study from elementary particles to our knowledge of elementary particles, and concludes that " the object of research is no longer nature itself, but man's investigation of nature." His comment on the statistical character of quantum theory, that " we cannot possibly assume that the future will see any basic changes in this field," has an all too familiar ring, and is fortunately contradicted by the earlier statement concerning " mathematically representable systems of concepts and laws applicable to certain realms of experience," that " we cannot expect these concepts and laws to be suitable for the subsequent description of new realms of experience."

It is undeniable that the nineteenty-century division between a dead objective nature on the one hand and a human observer on the other, each existing quite independently of the other, is no longer tenable, but, in the reviewer's opinion, it is a grave mistake to go to the other extreme and assert, with Professor Heisenberg, that " Science no longer confronts nature as an objective observer, but sees itself as an actor in this interplay between man and nature. The scientific method of analysing, explaining and classifying has become conscious of its limitations, which arise out of the fact that by its intervention science alters and re-fashions the object of investigation. In other words, method and object can no longer be separated." The division between observations and interpretation of observations is as sharp as the different nineteenth-century dicision was, and the interpreter does confront the observation as an independent agent. What has happened is a change in the subject-object duality from " nature-observer " to " observation-interpretation " and it is highly misleading to present it as though there were now no subject-object duality. This has been responsible for many illusory deductions by philosophers and theologians.

In commenting on this section I cannot refrain from quoting a description of modern science as " a specifically Christian form of Godlessness."

A discussion of classical and scientific education follows, which is full of interest and partly autobiographical, but not of special concern to physicists. I will therefore merely say that Professor Heisenberg sees Western culture as a whole, beginning with classical philosophy and developing into modern science. He therefore regards the classics-science rivalry as a false one. " It does not matter whether we prefer a classical or a scientific education. What alone matters is our unshakable faith in the West."

Finally—and this is the largest section of the book—a series of extracts is given from the writings of outstanding scientists during the last 350 years. Kepler, Galileo, Newton, Huygens (with interpolated notes on Gassendi, Boyle and Descartes), d'Alembert, the French Encyclopaedia, de la Mettrie, Ostwald, Hertz and de Broglie contribute to this symposium. Perhaps a clue to the choice is afforded by the author's general conclusion that "modern science, in its beginnngs, was characterized by a conscious modesty" which was "largely lost during the nineteenth century"; "to-day physics is undergoing a basic change, the most characteristic trait of which is a return to its original self-limitation."

There is a sense in which this is true, but I do not think it is quite the sense which the author intends. That, however, is a matter of opinion. What is more worthy of notice is a serious misinterpretation of some of the passages, which, indeed, is sometimes seen to be so from other quotations given elsewhere. For instance, we are told, in the account of Galileo's conception of science, that "by demonstration we mean the determination and explanation of the observed phenomena with respect to a given hypothesis," and "with Galileo . . . the properties of the phenomena to be determined are only investigated from the viewpoint of man-made hypotheses." This is profoundly wrong. By demonstration Galileo always meant purely mathematical demonstration; hypotheses played no part in his philosophy at all.

Again, Professor Heisenberg says that Newton "freed nature from an all-embracing God-relatedness." On the contrary, it was the omnipresence of God that Newton regarded as the guarantee of the absoluteness of space, and, as Professor Heisenberg quotes elsewhere, "to discourse of [God] from the appearance of things, does certainly belong to natural philosophy." Concerning gravity he writes: "Some had thought that the force of gravity was inherent in all bodies, others had suspected it; but Newton was the first and only one to demonstrate its existence by means of the phenomena themselves, and to give it a firm foundation through his great theory." This is based on Cotes's Preface to the second edition of the *Principia*. But Cotes was wrong: Newton emphatically did *not* regard gravity as inherent in bodies. Four pages further on the quotation is given: "Not that I affirm gravity to be essential to bodies: by their inherent forces I mean nothing but their inertial forces." And in a famous passage in a letter to Bentley, Newton wrote: "You sometimes speak of gravity as essential and inherent to matter. Pray do not ascribe that notion to me."

Such errors as these are very unfortunate in a historical account, and it is a great pity that the value of this section of the book is thus diminished.

HERBERT DINGLE.

PROCEEDINGS OF THE FIRST INTERNATIONAL CONFERENCE ON OPERATIONAL RESEARCH

Edited by D. J. Hughes

London: English Universities Press Ltd. Pp. viii + 526. Price 50s.

It is generally accepted that Operational Research arose largely as a result of war-time experience in the R.A.F. Scientists called into Fighter Command with the primary intention of using their technical knowledge and skill in maintaining and improving the operational use of radar equipment, found they could turn their scientific outlook to a much wider range of military problems where their purely technical experience was not of direct value. The value of this highly unorthodox source of advice became obvious to many high ranking military officers and operational research or analysis units eventually formed an inherent part of most military headquarters. A considerable degree of freedom was given to the individual scientists concerned and the scope of the research unit depended to a marked extent on the interests and outlook of the individuals it comprised. Very often these units were able to furnish Service chiefs with quantitative data on which their subsequent decisions could be partly based. Operational research is now considered an essential part of planning in the post war Services.

These unusual war-time experiences were of equal value to the scientists themselves, by enlarging their field of activity, allowing them to apply scientific methods in new fields divorced from laboratories and scientific equipment and to judge directly the value of the application to which their analyses led.

The war-time success of operational research encouraged the hope that it could also be applied in civilian life. The success of such investigations has, however, been less obvious, if only because of the greater difficulty of implementing their conclusions, the less spectacular fields in which they were applied and the prior existence of organizations already carrying out somewhat analogous work. In many branches there was also a lack of incentive in publishing the results of studies which might be of value to competing firms. Nevertheless,

several international societies have grown up to interchange information on operational research ; the volume under review comprises the papers delivered at the first international meeting held at Oxford in September, 1957. The meeting was organized by The Operational Research Society, United Kingdom, The Operations Research Society of America and the Institute of Management Sciences, and was attended by 250 delegates from 21 countries. The five sessions dealt with common theses, methodology, and applications. Summaries of operational research development in various countries were also presented. The range of subjects covered can be seen from a number of examples taken at random ; operational gaming, application of linear programming to large scale transportation problems ; queueing problems occurring in telephone and telegraph theory ; water storage policy in a simplified hydro-electric system ; traffic capacity of weaving sections of roundabouts. The most interesting feature in these and similar problems is the existence of common factors which form the basis of any future generalized theories of operational research.

A problem of considerable general interest is the place of operational research in the scientific field. According to Professor Morse of M.I.T. who opened the proceedings, operational research can only be considered as a member in its own right of the scientific family if it achieves a sufficiently unified method of approach which differentiates it from other branches of scientific activity. This aspect was discussed by a number of lecturers, Sir Charles Goodeve in particular dealing with the scientific method in so far as this applies to operational research. Operational research may perhaps be expected to lead eventually to general laws of a character somewhat analogous to the second law of thermodynamics, but in fields of human activities where the scientific method of abstraction of data, generalization, testing and resultant modification of the first generalization, has not so far been adequately applied. Certain aspects of economics may for example offer a suitable field for such investigations.

A further aspect of operational research which was discussed consists in selling the results of a study to the person ultimately responsible for using it. It is precisely in this field that conventional scientific methods differ most from those ound necessary in operational research.

The reader of an operational research report will often be ill-acquainted with scientific work but convinced of the general merit of science. He will often place too much reliance on the results of such work ignoring its limitation and qualifications (explicitly or implicitly stated by the scientists) and apply it in areas where the writer of the report would not consider it valid.

In operational research the time factor is often of major importance whereas in the more conventional types of research high accuracy is a more general requirement. Scientific research is subjected to laws of diminishing return which some research workers are often unwilling to admit. For many operational research problems a prediction offering a 90 per cent probability of being correct may be more valuable than one with a 95 per cent probability if the latter requires perhaps four times as long to achieve.

In many of the problems studied in operational research the amount of data is limited and cannot be readily increased. Operational research then attempts to assess the most likely results and their relative probability. Statistical analysis therefore plays a large part in this subject, so much so, that it is sometimes confused with operational research itself. Again simple models have been employed to represent a system being studied where more appropriate models could not be readily developed or analysed. The mathematical techniques used to follow the behaviour of the simpler model are often unjustifiably elaborate. This is one of the many dangers against which the operational research worker must guard himself. These criticisms are a healthy sign of growth in what is effectively becoming a new branch of science. In the opinion of the reviewer, there is little justification for attempts to restrict the scientific method primarily to the physical sciences ; its application to other human activities, in which deductive reasoning from general and largely unchecked principles often predominates, may provide a much-needed stimulus. It is to be hoped that the volume under review will receive a wide circulation in such circles ; the individual papers covered a sufficiently wide range for a selection at least to be read with interest by non-technical people. The editors and publishers are to be complimented on producing this excellent volume so rapidly after the date of the Conference.

It is customary for a reviewer to point out a few errors. In the volume it is deduced from a general principle somewhat analogous to the second law of thermodynamics that coals are not sent to Newcastle. This general proposition, or at least its application in the case of Newcastle is not valid, since the reviewer has himself seen open cast coal from Durham coal fields being transported by lorry in this very direction.

A. CHARLESBY.

PHYSICS

By H. Semat and R. Katz

New York : Rinehart and Co. Inc. Pp. xi + 927. Price $9.

Professor Semat's previous works are well known and his *Fundamentals of Physics*, on which the present book is to some extent based, is now in its third edition. *Physics*, however, contains much new material and over 200 new figures. It is not easy to place the book accurately in the syllabus of a British University course in physics because its scope is wide and overlaps the work of several undergraduate years. Broadly, its standard would be that of a first year course, with some leakage into the second year of study.

There are chapters on Mechanics, Heat, Wave Motion and Sound, Electricity and Magnetism, Light and Atomics and Nucleonics (the latter is perhaps a somewhat undignified designation). The book is well planned and carefully written and seems to have everything that a faithful student should, and usually tries to, absorb. The book is well bound and printed, perhaps unnecessarily so for these penurious days.

J. D. Craggs.

BASIC PHYSICS

By A. R. Meetham

London : Pergamon Press Ltd. Pp. xi + 144. Price 21s.

Dr. Meetham gives his aims in writing this book in the preface. He has in mind a statement of physics not far removed from the "A" level of the General Certificate of Education but tending in its escape from the historical context, and from slowly developing experimental experience, towards " Pure Physics," physics that is to say, free from the particular emphasis which derives from our specialized senses and from the accidents of our environment.

The first and immediate tribute is that, as one reads, one constantly finds oneself wondering about variations of presentation. This speculation is as a rule inconclusive, but it repeatedly draws attention to the care with which the form of the book has been shaped and the economical elegance of expression which will present an excellent example to many young readers.

It is a little surprising to find lens systems, even so far as the Galilean telescope, appearing in *Basic Physics*, but apart from this incursion the treatment, from wave motion at a quite early stage to heat and thermodynamics practically at the end, develops smoothly and consistently. One is frequently delighted by the illuminating directness of statement.

It is most difficult to imagine how this book would appear to the non-physicist, but the suspicion is that it is not meant for him and would prove overcondensed and insufficiently explanatory. The better physicists in the third year in the sixth form, and perhaps in the long vacation before entering the university, are likely to find most benefit, while any undergraduate physicist could read this book with pleasure and profit. This list could be extended, but it is enough that these and many others will find this fresh statement a stimulant to thought and an example of effective expression.

J. G. Wilson.

ELECTRICITY AND MAGNETISM

By C. G. Wilson, A.Inst.P.

London : English Universities Press Ltd. Pp. xiv + 548. Price 21s.

This book for Advanced and Scholarship level students provides a thorough groundwork, with clear exposition of principles and a good number of worked examples, suitable for sixth-formers intending to read physics at university. In addition to classical Electricity and Magnetism brief chapters on Cathode Rays, Electrons and Electron Physics ; X-Rays, Radioactivity and Atomic Structure ; and Nuclear Physics are included.

Rationalized m.k.s. units are used throughout, being ingeniously incorporated in a " traditional " approach via point charges and poles. An appendix deals with units and dimensions—it is surprising that so comprehensive a text does no more than mention absolute measurement of resistance and current. Problems appear to have been converted to rationalized m.k.s. units where appropriate, so that a student is given no opportunity to gain experience of calculations in any other system which he might, for instance, find in an examination paper.

The significance of " rectangular " B-H curves is not mentioned in the section on Permanent Magnet Materials and the use of two cold junctions with thermocouples in which neither metal is copper is not described. In a few cases the scale of a graph is not clear, e.g. the ordinate scale on page 342. There are but minor criticisms.

The book is well produced, reasonably priced, has clear diagrams and sufficient mathematical tables to be self-contained for the working of problems. The volume can be recommended as a good solid text for those already attracted to physics.

R. F. Y. Randall.

THE GENERAL PROPERTIES OF MATTER

By F. H. Newman and V. H. L. Searle.

London : Edward Arnold (Publishers) Ltd. Pp. xi +428. Fifth Edition. Price 32s. 6d.

The first edition of this textbook appeared as long ago as 1939 and the fourth thoroughly revised edition in 1948. The senior author, Professor Newman, unfortunately died in 1952, and it has been left to Dr. Searle alone to prepare the present fifth edition. The main change is the omission of the chapter on the production and measurement of low temperatures, the inclusion of which was a feature of the fourth edition. Dr. Searle quite correctly regards vacuum techniques as a technological subject and somewhat of a departure from the general theme of the textbook. Besides, several excellent books dealing with the subject have been published in recent years. The available space is utilized by Dr. Searle for a very desirable discussion of the special theory of relativity and elementary rheological principles.

Except for minor alterations and additions the rest of the contents remains unchanged. The text has a strong physical bias, but despite the many descriptions of experimental work and apparatus the physicist who prefers his physics described with a minimum of mathematics will find the book not easy to follow. The standard is that of the special honours degree student. Most of the discussions are devoted to the more classical and pre-1939 experimental and theoretical investigations, but reference is made in several cases to recent work even to as recently as 1955 or 1956. There can be no doubt that as a sound basic introduction to the understanding of the properties of matter of interest primarily to the physicist, but in addition, to a large extent to the engineer and chemist, Newman and Searle's textbook ranks in the first class, and all teaching institutions would be well advised to include its title not only in their library list but also in their lists of recommended textbooks for advanced student use.

S. WEINTROUB.

The Institute's Journals

PRINCIPAL CONTENTS OF THE JULY, 1958, ISSUES

Journal of Scientific Instruments: Volume 35, No. 7

A mechanical version of the Smith chart. By J. E. Knowles.

A modification of the Smith chart has been devised for the purpose of computing the transmission characteristics of non-absorbing optical or electrical multi-element filters. The conventional printed chart is replaced by a system of mechanical linkages together with a spiral computing element. The computer is more accurate, faster and less laborious to use than the orthodox chart.

An electrically-driven disk atomizer for high speeds of rotation. By D. J. Ryley.

Disk speeds ranging from 15,000–90,000 rev/min are attained by employing a variable speed d.c. motor/inductor alternator set and supplying the high-frequency output to a special induction motor carrying the disk. The apparatus is designed for continuous use at any speed and several protective devices permit unattended operation. The speed is measured by employing a diametral hole in the high-speed shaft to admit a light beam intermittently to a photocell. The resulting signal is amplified and fed to a standard frequency meter. Disk diameters are 2, 3, 4 and 5 cm.

An improved electromagnetic integrator. By A. J. Dyer, F.Inst.P.

An electromagnetic integrator designed for use with an instrument providing automatic measurement of natural evaporation is described. The integrator can integrate an electric signal of varying polarity and operates satisfactorily for currents down to 5 μA, and for electromotive forces down to 0·6 mV according to the type of coil used. The latter is wound on a light damping frame to ensure accurate integration together with rugged performance. As zero drift cannot occur with this type of integrator, it could be applied to problems for which long periods of integration are required, such as daily totals of net vertical solar radiation.

A compact and versatile fraction collector. By A. Snow, F. A. E. Porter and A. W. Tomalin.

A versatile fraction collector is described, which has many desirable features. The tubes are arranged spirally to conserve space, but each tube is brought in turn directly under the column in order to avoid mixing of successive drops in any connecting tube. The turn-table movement can be initiated without

difficulty by most devices used for determining sample size. The turn-table itself is easily replaceable and can be removed and used for storage.

A high voltage electron injector gun for synchrotrons. By E. W. V. Acton, A.Inst.P., and K. T. W. Milne.

An improved demountable electron injector gun designed primarily for use in 140 and 30 MeV synchrotons is described. The maximum energy of injection is 100 keV : facilities for the exploration of synchroton injection parameters are provided by micrometer control of both the radius and the injection angle of the beam. Both the cathode assembly and the anode are easily detachable for replacement and experimental purposes. Details of the gun design, its electrical characteristics, and the test apparatus are given.

A spectrograph attachment for high speed cameras. By D. P. C. Thackeray, A.Inst.P.

An attachment is described which will convert any camera into a spectrograph. Used with two typical high-speed cameras, time resolved spectrography of small sources is possible with a wavelength resolution of 3 Å at 5780 Å and resolution times down to less than 10^{-7} s. With a third camera, spectra of phenomena having a spatial dimension may be recorded at rates up to $1{\cdot}6 \times 10^{7}$ frames per second.

A simple, automatic, high-temperature thermal analysis apparatus. By S J. Lloyd and J. R. Murray.

A simple, automatic, high-temperature, high-vacuum thermal analysis apparatus is described. The sample, in a chamber which may be evacuated to better than 10^{-5} mm of mercury at 1000°C, is heated by a molybdenum element and the thermal arrests are indicated by the differential method. By placing the sample in direct contact with the thermo-couple hot-junction, solid state phase changes may be detected in samples as small as 0·3 g. Alternatively, samples of 20-80 g may be contained in crucibles and, using the arrangement, thermal arrests have been detected at temperatures up to 1550°C.

A method of obtaining accurate relative pressures in the range 20 to 200 mm of mercury. By G. A. Bottomley.

A refined visual method of accurately setting a mercury surface to a tungsten reference point is described in detail. The technique is applied in a differential manometer system to the establishment of a scale of relative pressures which is self-consistent to better than two parts in 100,000.

On numerical Fourier transformation, with special reference to Lipson-Beevers strips. By P. Fellgett.

Lipson-Beevers strips have been used in trials aimed at illustrating, in two special cases, the effect of rounding-off errors in numerical Fourier transformation. No evidence has been found to support the suggestion, sometimes put forward by false analogy with the Gibbs phenomenon, that such errors lead to systematically oscillatory residuals in the calculated ordinates.

A portable apparatus for recording the rate of clearance of radioactive sodium from human calf muscle. By L. Molyneux, J. Turnbull and D. N. Walder.

The apparatus consists of a halogen quenched Geiger-Muller counter which straps over the muscle to be tested and a light pack which is carried on the subject's back. The pack contains, in addition to batteries, the high tension generator for the counter, an electronic circuit in which alternate pulses from the counter are used to trigger a pulse generator, a wire recorder to record the pulses, and a ratemeter for monitoring purposes.

The record obtained on the wire recorder is subsequently played back in the laboratory, and after amplification the pulses are fed back into the monitoring ratemeter, the output from which is amplified and used to drive a recording milliammeter.

Jigs for making crystallographic wire models. By W. Hughes and C. A. Taylor, F.Inst.P.

Models made from stiff wire, in which the bends, junctions and free ends represent atomic centres, are often used in structure-determination and other crystallographic problems but are not easy to make accurately. Two jigs are described, one for making successive bends in a single piece of wire and the other for clamping wires in the correct relative orientations while soldering. They permit models to be made rapidly by a comparatively unskilled operator.

British Journal of Applied Physics: Volume 9, No. 7

Recent developments in electron microscopy. By V. E. Cosslett, F.Inst.P.

The new high resolution electron microscopes have made possible the imaging of molecular lattices directly and of metal lattices by the moire effects they give when superimposed. Dislocations can be observed both in such patterns and also in thin metal foils, in which their motion can be followed by electron cinephotography. In biology the trend is towards critical examination of the various stages in cutting thin sections, fixation, dehydration embedding and sectioning —to control artefacts and to improve contrast. For this purpose the use of compounds of heavy elements as " electron stains " is being actively explored. Their value is illustrated by a study of the localization of nucleic acid in the cell nucleus and in sperm. Of two new departures in instrument design the first is the development of scanning electron microscopes :

recording the scattered electrons shows the surface relief of solid specimens, recording the emitted characteristic X-rays shows the distribution of a particular element. In the second place, electron interference phenomena have been studied in some detail and the first tentative results obtained with a type of electron interference microscope, which would allow the measurement of inner potential as well as the thickness of very thin films.

Thermionic and related properties of calcium oxide. By B. Hopkins and Prof. F. A. Vick, F.Inst.P.

Using proble diodes, changes in thermionic emission and conductivity of cathodes of calcium oxide on nickel have been followed during activation and poisoning. The effects of material liberated from the anode have been studied. The mean nominal thermionic work function is 1·69 eV for the fully activated cathode (from the slope of a Richardson line), with an estimated temperature coefficient of 1·1 mV/° K.
Three regions of conductivity have been found ; the activation energies for the two at the lowest temperatures are 1·3 eV and 0·81 eV. The current-voltage characteristics between the cathode base and probe show two types of curvature, one dominant above 625° K and the other below this temperature. A linear relation extending over four orders of magnitude between the logarithms of emission and of the conductivity has been observed during activation. Assuming that semi-conductor theory may be applied the mean value of the surface work function is calculated to be 0·7 eV.

The analysis of particle counts by the spray-drop method. By K. I. Williamson, A.Inst.P., and W. B. Taylor.

The spray-drop method is used in electron microscopy to determine the concentration of particles in an unknown suspension. The usual analysis procedure is criticized and a better one is given and justified. A test is included to check the presence of variations in the data due to causes other than purely random ones, e.g. inadequate mixing. The test is applied to artificial sets of data to illustrate its usefulness.

The chromatic field aberrations and their correction in a three lens reflexion electron microscope. By D. H. Page.

The coefficient of the chromatic difference in magnification for a three lens reflexion electron microscope is found to be a function of the focal length of the intermediate lens and the axial position of the objective aperture. A condition is shown to exist for which the coefficient vanishes and the aberration is then corrected. The coefficient of the chromatic difference in rotation is proportional to the overall rotation of the image and can be minimized by exciting lenses in opposition. The effect on the image of a misalinement of the objective aperature is also dealt with. Tolerances in the settings of the correction conditions for the aberrations and in the alinement of the objective aperture are calculated for typical values of the energy spread in the electron beam at low and high angles of beam deviation.

Solid angle corrections to single scattering. By F. F. Heymann and P. I. P. Kalmus.

Corrections relating the average intensity of particles scattered into an aperture of finite solid angle to the intensity at the centre of the aperture are given for rectangular and circular apertures. The dependence of the corrections on aperture shape and scattering angle is calculated for electrons scattered according to the simple Mott formula.

New techniques for the study of bitter figures. By D. J. Craik and P. M. Griffiths.

New techniques for the study of Bitter figures by optical and electron microscopy are described. In the case of the optical microscope an improved resolution and clarity is achieved. The method for the electron microscope is a modification of that previously published. Examples of the use of the methods are given.

A photoelastic technique for strain measurement on flat aluminium alloy surfaces. By H. Fessler and D. J. Haines.

A technique is described for bonding layers of an epoxy resin on flat aluminium alloy surfaces. Joints which withstood more than 4% strain were obtained by carefully controlled preparation of the metal and the adhesive. Initial birefringence was avoided by curing the joint without external pressure and by slow cooling after finish curing at 65°C.
The different types of reflexion polariscopes are discussed, the strain fringe values for the reflecting layer and for the adhesive are estimated. A test of a lug describes an application of the method.

Stability and convergence limitations on the use of analogue computors with resistance network analogues. By M. E. Fisher.

Various practical methods have been described for solving partial differential equations such as $\nabla^2\varphi = f(\varphi)$ by means of resistance networks coupled to electronic analogue computing units. These schemes are investigated mathematically to determine the conditions for which the computer remains stable, or for which the successive approximations converge to the required solution. It is shown that all the methods will break down if the gradient $df/d\varphi$ is negative and sufficiently large to cause the correct solution to be " wave-like."

The performance of infra-red photoconductive cells. By D. H. Roberts and B. L. H. Wilson.

Expressions are derived for the responsivity and signal/noise ratio of photoconductive detectors under various conditions of noise limitation. These are used to show the basis for McAlister's relation, $E.N.I./\tau \simeq K$, and also the reasons for its failure to apply in the case of lead selenide cells. Experimental data obtained on lead selenide cells are presented, and a simple model for the flicker noise is shown to be consistent with the experimental observations.

CENTRAL ELECTRICITY GENERATING BOARD

NUCLEAR DEVELOPMENT BRANCH, HEADQUARTERS, LONDON

invite applications for the following appointments :—

(*a*) A PHYSICIST OR MATHEMATICIAN with a good honours degree to take charge of a section dealing with nuclear reactor core calculations, including reactor kinetics and fuel cycling.
Experience in the type of work involved or the possession of a higher degree would be an advantage.
The salary will be on a scale within the range £2,160 – £2,960 per annum, according to duties and responsibilities.

(*b*) PHYSICISTS AND MATHEMATICIANS with a good honours degree to work on various aspects of nuclear reactor core calculations, including reactor kinetics and fuel cycling.
Some experience of nuclear physics and/or reactor technology would be an advantage, but specialized training may be given in suitable cases. Salaries will be on scales within the following ranges, according to qualifications and experience :—£590 – £1,315 p.a., £1,210 – £1,795 p.a., or £1,540 – £2,005 p.a.

(*c*) A PHYSICIST to study the physical aspects of radiation damage, particularly to the graphite core and structure of nuclear reactors. This post should be of particular interest to solid-state physicists with a good honours degree and a number of years' post-graduate experience. In the first instance, the successful candidate will work at Headquarters in London, but he may ultimately be attached to the Board's laboratories. The salary will be on a scale within the range £1,490 – £1,945 p.a., plus London allowance where appropriate.

Application forms obtainable from S. S. Scott, Chief Personnel Officer, Central Electricity Generating Board, 24/30 Holborn, London, E.C.1, should be completed and returned by August 30. Please mark envelopes "Confidential." Quoting reference PB/211 also specific vacancy being applied for.

FLUIDIZED BEDS. Two PHYSICISTS are required by the British Rayon Research Association to undertake research and development work into the heat transfer properties of fluidized beds of particles. The physics of fluidized systems is a largely unexplored field and there are good prospects for capable research workers. The minimum qualifications for both posts is a good honours degree, but for one of them a Ph.D. graduate would be preferred. The F.S.S.U. scheme is in operation and annual leave is not less than three weeks.
Apply to the Secretary, British Rayon Research Association, Heald Green Laboratories, Wythenshawe, Manchester 22, quoting ref. M.D.4.

THE UNIVERSITY OF SHEFFIELD

Applications are invited for a United Steel Companies POSTGRADUATE RESEARCH BURSARY in the department of fuel technology and chemical engineering for work on the physics and chemistry of flames with recirculation. Available for the coming session, preferably for an honours physicist who wishes to apply fundamental research to practical problems. Value £300 per annum plus fees. Applications should be sent to the Registrar as soon as possible.

Ministry of Supply Research and Development Establishments at Farnborough (Hants), Bedford, Aberporth, Llanbedr (Wales), Sevenoaks (Kent), Waltham Abbey (Essex), Harefield (Middlesex), Risley (Lancs.), require (*a*) ASSISTANT EXPERIMENTAL OFFICERS (minimum age 18) and (*b*) EXPERIMENTAL OFFICERS (men and women) for experimental work in physics, electronics, electrical engineering, chemistry or mathematics.
Qualifications : H.S.C., G.C.E. (A.L.), H.N.C., pass degree or equivalent. Salaries (men) : (*a*) £370 (age 18) – £685 (age 26) – £850 ; (*b*) £920 – £1,190. Applicants should state preferred fields of work and location. Forms from M.L.N.S. Technical and Scientific Register (K), 26 King Street, London, S.W.1, quoting A.233/8A.

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NORTHERN POLYTECHNIC
Session 1958-59
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